Earthquake prediction – Any hope?*

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Earthquakes seem to occur at frequent but irregular intervals all over the world with severity that affect human habitation resulting in enormous destruction to life and property. Within this year several severe earthquakes have rocked Euro-Asean region; the Chamoli earthquake in North India followed by repeated onslaughts in Turkey and recently in Taiwan are fresh in our memory. Is there a possibility that one could have predicted these earthquakes precisely? What is the current status of forecasting, if possible, of an earthquake of, say, magnitude of 5.0 and above on the Richter scale, pinpointing the time and place where it is likely to strike? These are questions that occur naturally to help alleviate suffering, death and destruction. 'Nature' conducted a debate on this issue recently among several specialists engaged in Earth Sciences, study of seismogenesis of earthquakes and others. This article is a brief summary of the debate.

DURING this decade several minor and major earthquakes have occurred in India; the major ones have resulted in loss of human life and property. The recent one in Chamoli District of Uttar Pradesh in March 1999 has once again affected a vast population. The affected people have been in a state of shock as the tremors due to recurring shocks continue.

Amongst natural catastrophes, earthquakes seem to occur suddenly without any warning whatsoever and seem to be unpredictable. On the other hand, catastrophes like tornadoes, floods, volcano eruptions, etc. seem to be predictable to some extent. Reliable and accurate predictions of earthquakes would circumvent a lot of human misery, hardship and death.

Recently a debate was underway in *Nature* (website www.nature.com) concerning the problem of predictability of earthquakes. This article is based on the debate and provides essentially a summary of the debate.

The Earth is a dynamic planet with its associated tectonic plate movements, subducting plates, oceanic currents, fuming volcanoes, tidal motions, etc. The system is a complex one, undergoing continuous change and evolution. The hope for earthquake prediction of any modest size at any location and time has to be based on reliable physical models that take into account a variety of factors and data as well as past history. **Ian Main** (Department of Geology and Geophysics, University of Edinburgh, UK), the moderator for *Nature*'s debate states that 'even simple nonlinear systems can exhibit chaotic behaviour, whereas more 'complex' nonlinear systems with lots of interacting elements, can produce remarkable statistical stability while retaining an inherently random component'. To set the debate-ball in motion, Main raised the question as to 'whether the accurate, reliable prediction of individual earthquakes is a realistic scientific goal? If not, how far should we go in attempting to assess the predictability of the earthquake generation process?'

It is stated that recent research and observations have shown that seismogenesis of earthquakes is not totally random – spatial and temporal localization of earthquakes seem to contradict a general random occurrence in space and time. A certain amount of determinism and predictability is expected based on considerations like fault morphology, earthquake frequency–magnitude distribution, etc.

Main has proposed four scales of prediction of earthquakes. They are (a) time-independent hazards, (b) time-dependent hazards, (c) earthquake forecasting and (d) deterministic earthquake prediction. In going through this 'sliding scale' of earthquake prediction, he proceeds from a scenario of assuming that earthquakes are essentially a random process in time but use past records of earthquakes, geological features and seismic characteristics to constrain future 'seismic hazards' at one end of the scale (useful in developing building codes, land use, etc.) to a scenario where earthquakes are inherently predictable to be able to evacuate people from *likely sites* of occurrence of an earthquake in advance at a *certain time* by assessing *the magnitude* of the impending earthquake, the characteristics (site, time and magnitude) being known within narrow limits. Unlike forecasting, deterministic earthquake prediction is not probabilistic. Long-term, 'forecasts' of hazards or earthquakes are not classified as 'predictions'.

However, it is still believed that predictions would only be probabilistic to the extent that an earthquake of a certain magnitude will cluster spatio-temporally following a precursor of a certain magnitude. Even such 'forecasts' would be helpful guides to the Hazard Management Agencies. But now the emphasis shifts from the prediction of a major earthquake to a precise, unambiguous, identification of precursors. Forecasting or prediction of an

*A status summary based on a recent debate.

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earthquake is a difficult job currently because it should not lead to false alarms lest public loose confidence in such forecasts and do not pay attention to preventable catastrophes. On the other hand, a conservative forecast may miss warning the public in good time; hence degree of predictability comes into focus of the debate.

Robert Geller (Department of Earth and Planetary Physics, Tokyo University, Japan) begins his discussion by stating that, over the past 100 years, 'great efforts, all unsuccessful, have been made to find hypothetical precursors'. In Geller's opinion, 'predictions' have been based on unreliable scientific approaches that are either hypothetical or empirical, looking out for a 'candidate-precursor' or 'anomalous signals' fraught with ambiguity. 'Retrofitting' of observations to possible precursors is not a desirable approach.

Heterogeneity of earth's crust, small variations in initial conditions of stress release, nonlinear interaction between faults in the crust are believed to hamper development of this field. Geller says 'earthquake prediction seems to be the alchemy of our times' since it attracts attention of scientists as well as that of the general public owing to 'combination of their extreme difficulty and great potential reward' due to sociological rather than scientific reasons. In view of this he argues that 'there is no need for enormous funds for specialized agencies or research programmes for earthquake prediction'; however, he supports research for long-term seismic hazard estimates, real-time seismology and well co-ordinated seismological research.

According to **Max Wyss** (Geophysical Institute, University of Alaska, USA), contrary to the suggestion of Main, 'massive efforts of rigorous science to understand the nucleation process of earthquake are not made in any country'. He goes on to state that there are two small groups of people: (a) those who argue that earthquake prediction is impossible and (b) those who argue that the problem of earthquake prediction is already solved. Both these groups often distort the facts. According to him, 'earthquakes do not occur "suddenly" as is stated by some, that it is therefore impossible to predict the quakes; 10 to 30% of them are preceded by foreshocks during the week before their occurrence; some have year-long preactivity, some are preceded by release of energy for years and some are by seismic quiescence. These have been used in predicting *some* (emphasis mine) earthquakes correctly'.

So the debate has two camp-followers – one who argue on the acceptability of long-term time-independent hazard estimates and the other who argue on the futility of current short-time earthquake prediction. The prediction research is plagued, in the opinion of Wyss, with scientifically weak work which find their way into journals and work and statements made by scientifically unqualified publicity seekers. Therefore, he says that 'we must learn how to conduct rigorous, quantitative prediction research in spite of the distractions generated by unqualified people'.

Wyss is pessimistic about earthquake prediction in the near future but optimistic about long-term predictions that improve 'our ability to predict some earthquakes in favourable areas, although not often with time windows as short as demanded by the moderator'.

Pascal Bernard (Institute de Physique du Globe de Paris, France) enlarges the scope of the debate from merely focusing attention on earthquake prediction to paying attention and studying the whole set of crustal instabilities.

A variety of crustal instabilities have been observed: (i) aseismic fault slips involving only the upper few km of earth's surface over periods of hours to a few days; (ii) silent and slow earthquakes of long periods involving low frequency slip events on time scales of minutes; (iii) fluid migration instabilities; (iv) seismicity of non-Poisson nature at clusters of earthquakes lasting from hours to years over hundreds of meters to hundreds of km; even seismic quiescence is noted over time scales of years; (v) size and roughness of fault segments and sizes of earthquakes seeming to follow power-law distribution. Two standard model approaches are: Model I, a model of seismogenesis in which processes (i) to (iv) are recognized as precursors to large earthquakes and Model II, in which process (v) is the basis for self-organized critical models for the crust or similar models in which one associates earthquakes to be inherently unpredictable in size, space and time. Both these models do not seem to be successful so far.

According to Bernard, significant progress in understanding the whole set of crustal transients and their interactions and coupling, perhaps based on a model of self-organized criticality[1], is required before questions about earthquake predictions can be considered realistically. In Bernard's opinion, earthquake prediction should not be a scientific target at this stage as it is more relevant to study and understand crustal transients and this could be a realistic scientific goal in the near future.

Andrew Michael (USGS, USA) has discussed details of the four scales of predictions proposed by Main in his introductory remarks to the debate. He believes that one can easily predict the behaviour of populations of earthquakes though not that of individual ones. As far as time-independent hazard estimation is concerned, he opines that one can calculate estimates of average earthquake rates and such results are used. Regarding time-dependent earthquake hazard estimates, temporal and spatial earthquake clustering can lead to improvements over the time-independent hazard estimates. Probabilistic estimates, that is, forecasts of aftershock rates are useful. If one were to recognize foreshocks, perhaps one can predict aftershocks but 'no one is able to identify which earthquakes are foreshocks'. Earthquake clustering is currently used as a tool of time-dependent hazard estimation. Michael does not agree with Main that efforts to find precursors have been significant; right now one cannot rule out existence of precursors on the basis of lack of observations as only limited efforts have been made in this area. Regarding earthquake prediction, he expresses hope that our knowledge may improve with new observations.

In conclusion, Michael believes that scientists are making societally useful predictions based on both the behaviour of the population of earthquakes and the individual events. He says that the progress in this field might be difficult but one should heed Peter Medawar's advice[2] – 'No kind of prediction is more obviously mistaken or more dramatically falsified than that which declares that something which is possible in principle will never or can never happen'.

There are arguments, according to Christofer Scholz (Lamont-Dohertz Earth Observatory, Columbia University, USA), that predictions are intrinsically impossible for two reasons: 'one, because the earth is in a state of self-organized criticality, everywhere near the rupture point, so that earthquakes of any size can occur randomly anywhere any time'. 'The second reason for impossibility of earthquake predictions is based on the conjecture that an earthquake cannot "know" how big it will become because that depends on initial conditions'. Therefore, even if nucleation can be sensed, say, based on friction theory, as mentioned by Michael, the ultimate magnitude of the earthquake is not predictable.

Based on these reasons, one may conclude that we may not have a method for making short-time prediction but it is not justified to assert that it is impossible. Long-term earthquake predictions seem to be working. With a decade of lead time, it can guide engineering and emergency planning measures to mitigate the effects of earthquakes. Similarly, post-earthquake seismicity may also help in identifying faults that may have been brought closer to failure by the preceding earthquake. Although short-term earthquake predictions may not be within immediate reach, there is hope that this may be aided by the networking of global positioning system networks that are being set up around the world and by the satellite radar interferometry which may allow one to view evolution of strain fields in space and time.

'For a prediction to be successful, the probability of occurrence in time-interval and space region must be specified in advance, as must be the lower magnitude. However, we must guard against self-indulgence. If the time and space windows are made too broad and the lower magnitude is made too low, then we can increase the probability of success up to 100% without any serious efforts on our part, says **Leon Knopoff** (Institute of Geophysics and Planetary Physics, UCLA, USA). According to him, the societal needs for earthquake predictions are not universal. In the developed countries, lead times of earthquake predictions of even years is considered useful because this can lead to useful changes in codes/practices of engineering design, construction and the like. On the other hand, in the developing countries, lead times should be of the order of a few days to help the local and other governing bodies to help evacuate people from hazardous zones. One must have enough time to marshall men, machines, materials and other resources for such a purpose.

Knopoff particularly draws attention to the fact that the problem of earthquake prediction is not statistical in nature to go for probabilistic estimate of the earthquake. 'There have been too few large enough events in any small area in the past century to be able to define probabilities of the largest events sufficiently accurately.'

He mentions two ways to proceed: 'One is to proceed by the study of the intervals between earthquakes in the region in this magnitude scale; provided earthquakes are periodic, the problem is solved'. However, even in the best of cases of large data availability in paleoseismicity as in San Andreas Fault, it is observed that the interval times for the strongest earthquakes at one site have large variability. The interval time distribution itself is not known with the result that 'a long duration since the last occurrence is no guarantee that the next event is imminent; the next event could be farther in the future as Ian Main has also noted'[3].

The second approach is to search for the immediate precursors of various types (foreshocks, tilt, radon emanation, electrical precursors, etc.) of the strong earthquakes. Generally, this approach has not been successful so far. He points out several arguments against the applicability of self-organized criticality for the earthquake prediction problem: (a) faults and fault systems are inhomogeneous, (b) seismicity at almost all scales is absent from most faults, before any large earthquake on that fault, and (c) there is no evidence for long-range correlations of the stress field before earthquakes.

Knopoff opines that the development of a new physics-based theory of the precursory processes is the need of the hour, grappling the complex geometry of the faults and fault systems and build a model based on non-elastic deformation under high stress before fracture so that one can detect and identify precursors.

At this stage, in response to the debate that has already ensued, **Geller** opposes extensive funding of earthquake prediction research based on the arguments advanced by Wyss. He says that objective statistical testing of data and of stated hypothesis is important to resolve this question of predictions based on a variety of precursors. Even about long-term seismicity forecasts, he questions the reliability and accuracy of methods adopted; in fact, the forecasts based on seismicity gaps have not 'outperformed random chance' and there is a running debate on this issue. Countering the claim of long-term forecasts as being successful as put forth by Scholz, Geller states that 'even if the claim of "successful" were warranted in case of 1989 Loma Prieta, California earthquake, this appears to be a classic example of the gambler's fallacy of picking one possibly atypical sample out of a much larger data set'. 'The only way', according to him 'to avoid such problems in the future is for forecasters and independent evaluators to thoroughly thrash out all the ground rules at the time forecast is issued, before seismicity is known.'

He goes on to say that 'While we should be reluctant to recommend that governing bodies take strong action on the basis of long-term forecasts, there is ironically, a risk that the authorities in region for which long-term forecasts have not been issued may become complacent!!'

There is no better way to summarize Geller than quote more from his contribution itself: 'Rather than debating whether or not reliable and accurate earthquake prediction is possible, we should instead be debating the extent to which earthquake occurrence is stochastic. . . . Efforts at deterministic prediction seem unwarranted. In view of the lack of proven forecasting methods, scientists should exercise caution in issuing public warnings regarding future seismic hazards. Finally, prediction proponents should refrain from using the argument that prediction has not yet been proven to be impossible as justification for prediction research.'

In **Per Bak's** (Physics Department, Neils Bohr Institute, Copenhagen) communication, he says that 'simple mathematical modelling in comparison with empirical observations indicates that we are dealing with a self-organized criticality phenomenon. These include, as stated by Bernard, power law behaviour distribution of earthquake sizes and fractal, power law distribution of fault segments mimicking the highly inhomogeneous worldwide distribution of faults and fault-zones'. Self-organized criticality is the only model that reproduces the observation of clustering in space and time of earthquakes. According to this model, the waiting time distribution between earthquakes of a given size is given by T^{α} with $\alpha \sim 1.5$. Bak concludes by stating that 'if the crust of the earth is in a self-organized criticality state, there is a bleak future for individual earthquake prediction. On the other hand, the consequences of spatio-temporal correlation function for time-dependent hazard calculations have so far not been fully exploited!'

David D. Jackson (Southern California Earthquake Center, UCLA, USA) says 'It defies definition' in answer to the question 'What is meant by earthquake prediction?' Although evacuation is not generally envisaged as a response to earthquake prediction, when an earthquake prediction is made it only means that an earthquake is expected to occur with a substantially high probability and exceptional response is justified.

Time-independent hazards can be estimated quite well, for example, one can say that Japan is more prone to earthquakes than Germany but the precision of this estimate is rather limited in the sense that one cannot say that Japan is more or less prone to earthquakes compared to New Zealand.

On the other hand, time-dependent hazards cannot be specified. The two approaches namely clustering models and seismic gap theory contradict one another. Clustering models predict that not only aftershocks cluster but very large main shocks also cluster. On the other hand, seismic gap model asserts that 'large quasi-periodic earthquakes deplete stress energy, preventing future earthquakes nearby until the stress is restored'. Earthquakes seem to follow both these types of predictions. But in the words of Jackson, 'the seismic gap model has failed every prospective test'. Regarding the attempts at earthquake forecasting, he says 'given the bleak record in earthquake forecasting, there is no prospect of deterministic earthquake prediction in the near future', echoing Bak's sentiments, albeit from a different perspective.

Jackson discusses the nature of difficulty in earthquake prediction. As already stated, earthquake prediction based on precursors arising and belonging to diverse phenomena is an empirical exercise. The signatures of such phenomena vary from place to place and time to time. In his words, 'monitoring these phenomena without complete understanding is courting trouble; monitoring them properly is a huge effort with only a remote connection to earthquakes'. The only other strategy is to depend on detailed Earthquake Physics, which is still not ripe. 'To forecast' instead of 'to predict' is itself 'a solid achievement', according to Jackson, given that the knowledge of strain accumulation and mechanical properties of the crust (which are yet to be monitored) do help to predict some properties. In this scenario, Jackson looks at what is possible right now. Commenting on the debate regarding self-organized criticality, he points out that, 'some argue that earthquakes possess a property known as self-organized criticality, so earthquakes cannot be predicted because seismological regions are always in a critical state . . . others argue that self-organized criticality comes and goes and that outward signs of self-organized criticality (such as frequent medium earthquakes) provide the clue that a big earthquakes is due. If self-organized criticality comes and goes, it is not clear how to recognize it'. Reverting to the question posed by Main, in his opening remarks, Jackson says 'the important question is not whether earthquake prediction is possible but whether it is easy. Otherwise it is not a realistic goal now . . .'.

David D. Bowman and **Charles G. Sammis** (Department of Earth Sciences, UCLA, USA) discuss the role of 'intermediate-term' earthquake prediction. Although short-term predictions may be largely unsuccessful, one should pay attention to forecasts in the year-to-decade scale, as such forecasts are still useful. Once again the self-organized criticality state of the crust seems to come as a stumbling block even in this exercise. They refer to self-organized criticality as follows: 'The critical state is defined as a system in which the stress field is correlated at all scales, meaning that at any time there is an equal probability that an event will grow to any size. The self-organized criticality will keep the system at the critical state relying on a constant driving state .... Because of this, earthquakes are unpredictable. However, this is contradicted by recent evolution of the stress field after earthquakes.' A 'shadow' in the static stress field is produced that seems to inhibit earthquakes for many years after a major event. Scholz has referred to this concept earlier, treating this as equivalent to 'seismic-gap' hypothesis. The hypothesis referred to as 'intermittent criticality' refers to the model that describes how the system emerges from the stress shadows: 'A large regional earthquake is the end result of a process in which the stress field becomes correlated over increasingly long scale-lengths, resulting in the system approaching a critical state. The scale over which the stress field is correlated sets the size of the largest earthquake that can be expected at that time .... This large event then reduces the correlation length, moving the system away from the critical state on its associated network, creating a period of relative quiescence, after which the process repeats'. They conclude the discussion by cautioning against overstating one's claims, as these forecasts can change through times like weather forecasts.

Andrew Michael joins the debate at this stage with a rather moderate view: 'The public should neither expect to be saved from (an impending) calamity by such predictions nor support research based on this expectation. The research may still bring benefits even if they are less spectacular than the vision of huge populations in mass exodus.' He goes on to make a rather interesting observation: 'Even knowing that earthquake prediction is impossible would be useful... to deal with problems posed by less scientific approaches. However, our current understanding of earthquake physics cannot prove this point'. From the debate so far, discussion relating to self-organized criticality models has neither clearly related the models to their implication to earthquake prediction nor to earthquakes.

Geller's response at this stage of the debate takes on a more virulent aggressive posture. He compares the controversies present in the debate to those that existed in this decade regarding 'cold fusion'. He says: 'In all episodes of "pathological science" there are some credentialed scientists who hold out indefinitely in support of generally discredited theories. ... Perhaps it is time to consider whether the prediction debate has reached a point when the mainstream scientific community decides that one side or the other has nothing new to say and treats this discussion as effectively closed, barring truly new data'.

Stuart Crampin (Centre for Reservoir Geoscience, University of Edinburgh, UK) says, 'Earthquake prediction is not just a difficult subject where more knowledge or funding is required; it is out of our reach by astronomical sized factors'. He believes that neither monitoring precursors nor understanding nucleation processes and similar other approaches are likely to succeed. He illustrates the nature of complexity of earthquakes by listing a number of factors like magnitude and direction of the stress field, shape of fault planes, presence or absence of fluids, nature of fluids and their pressure, temperature, state of earth and ocean tides, local geology, etc. that are responsible for the variation and complexity of earthquakes. In his opinion, there is no hope whatsoever of earthquake prediction, that 'such hopes are futile and not worth wasting time or spending money on'.

However, Crampin suggests that monitoring the build-up of stress may be adopted as a third strategy beyond the two others suggested by Jackson, namely detecting precursors and detailed modelling of earthquake physics. According to him, there is mounting evidence that changes in seismic shear-wave splitting (seismic birefringence) can build up the necessary stress before an earthquake can occur.

Zhongliang Wu (Institute of Geophysics, China Seismological Bureau) has drawn attention to the importance of classification of earthquakes by the physics of their seismic source and cataloguing, an important aspect noted by R. Hoemes some 120 years ago in 1878; not all earthquakes within a magnitude–space–time range can be treated on the same footing when it comes to earthquake prediction hypotheses testing. Wu goes on to state 'from this point of view, it is too early to accept the conclusions that the search for earthquake precursors has proved fruitless and earthquakes cannot be predicted. At the other extreme, the ignorance of some proponents of earthquake prediction to the difference between earthquakes and the attempts to "improve" the performance or the proposed precursors have led to too many predictions leading in turn to too many meaningless false-alarms'.

Didier Sornette (Institute of Geophysics and Planetary Physics, UCLA, USA and National Center for Scientific Research, LPMC, CNRS, France) made a post-deadline contribution to the debate. In his presentation, he states that the multidisciplinary activity of earthquake prediction has not made use of the full potential of various disciplines such as artificial intelligence, supercomputational modelling, large-scale monitoring of full spectrum of physical characteristics along with traditional seismic and geological approaches. He takes exception to the reference to earthquake prediction as 'the alchemy of our times' made by Geller earlier in the debate. Comparing with current status of weather forecasting, he says – 'We are very far behind meteorology for two reasons: We still have very limited precise quantitative measurements of the many parameters involved. Secondly, the physical parameters underlying earthquakes are much more intricate and interwoven and we do not have a fundamental Navier–Stokes equation for the crust'. Amongst other comments he has made to various other issues that have cropped up in the debate, the ones relating to criticality and self-organized criticality are interesting. 'The most interesting aspect of self-organized criticality is its prediction that the stress field should exhibit long-range spatial correlations as well as important amplitude fluctuations. Spatial correlation of stress–stress correlation around the average stress is long-range and decays as a power law with distance . . . This supports the view developed by Crampin that stress monitoring on large-scale may be a good strategy for earthquake predictions.' Sornette refers to the use of criticality and self-organized criticality as two different but likely coexisting concepts in the underlying statistical physics of earthquakes.

The last part of the debate contains the 'Concluding remarks' by **Ian Main**, the moderator of the debate. I shall not summarize this but shall draw attention to a few statements that Main makes: (a) 'It would be hard to devise any model which can truly account for the enormous complexity of the earth'. (b) 'The debate has highlighted both a degree of consensus and a degree of continuing controversy within the thorny subject of earthquake prediction'. Most importantly, (c) 'In the end, it is not earthquakes themselves which kill people, it is the collapse of man-made structures which does most of the damage. While we continue to explore the degree of predictability of earthquakes on rigorous observational, statistical and theoretical grounds, we should therefore not lose sight of the fact that the best way of preparing for the inevitable remains in the development of land use plans, and building and infrastructure design codes to mitigate their worst effects'.

When this write-up was under preparation, I came across an article[4] by **R. N. Iyengar** (Central Building Research Institute, Roorkee, India) in which he has drawn attention to the importance of historical records for estimating the seismic hazard in a certain region. As far as records for the Indian subcontinent are concerned, he says that reliable data are available only for the past 200 years. Iyengar's work has led to identifying some 20 earthquakes in the medieval period. In the article referred to above, he says 'this does not mean that our ancients were not fascinated or not affected by earthquakes. The *Vedas*, *Puranas* and the *epics* contain many references to earthquakes and allied phenomena... From amongst the writings of persons who were acclaimed as scholars of their times, two are available in print, namely the *Brihit Samhita* of Varaha Mihira (5th century AD) and the *Adbhuta Sagara* of Ballal Sena (10th century AD)'. After detailing some contents from these works, Iyengar concludes by stating 'the information so far available is very little, but valuable. It is quite likely that there are many Sanskrit manuscripts with scientific information yet to be published. It is hoped that the persons responsible for preparing seismic zonation maps of the country, will study the relevant literature written prior to the colonial period before drawing conclusions on the subject'.

Prediction of earthquakes has been a frustrating field of research[5]. Unlike physical phenomena, geophysical phenomena are a function of local geology, in this context, **Sankaran**[6] points out that 'In India, we have chronic seismic areas along the Himalayas, which lie well on the global earthquake belt, which also marks regions of plate boundaries. While events in the proximity or along this belt (like the ones in Himachal Pradesh or Uttar Pradesh or Bihar) are to some extent likely to provide clues for long-term predictions, the intra-plate quakes, that are far away from the plate boundaries, that have been rocking parts of Central India and the Deccan Plateau have been enigmas, particularly since these regions are long considered parts of a stable single block. As to the occurrences in the latter areas may possibly lie in the current view put forward by a team of earth scientists from Lucknow that India is a mosaic of four blocks, instead of a single block considered hitherto, welded together along the suture zones (the Aravallis in north-west India, Satpura in central India and Eastern Ghats in the south) all of them potential sites for stress build-up and hence likely to trigger earthquakes in these regions. Then, there are areas where large storage of water by way of dams, etc. permit seepage of water into fault planes to induce slippage and precipitate earthquakes'. In Sankaran's opinion, 'in India earthquake research should be geared to look for the peculiarities of each candidate site to establish viable precursors for the unique geological settings at that place'. This may not be able to provide short-term forecasts, but may help, as **Ian Main** has observed towards the end, 'to design suitable infrastructure to minimize their catastrophic impact. The latter approach to earthquake problem is more important than earthquake prediction research, particularly for our country with many areas having fairly high density of life and property where even a minor earthquake can result in a large-scale disaster due to inappropriate structures'.

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